The (Surplus) Value of Scientific Communication

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scientific communication; surplus value; Elias, N.; Bourdieu, P.; Merton, R.K.; Matthew effect; sociology of scientific communication

Abstract

In research on scientific communication there are above all theory-less and formal technical/ natural scientific models of scientific communication. These are juxtaposed to social-scientific, power-sensitive models (Elias, Bourdieu, Merton). The (surplus) value of scientific communication can be variously understood: either as inherent surplus values in the sense of potential effects of stimulation, synergy, critique, quality control; or as symbolic surplus value in the sense of symbolic capital (Bourdieu) which is adapted by scientists or respectively groups of scientists and so further reinforces the social disparities in the sciences with which they have contact (Matthew effect).

Contents

1 On the shoulders of giants and dwarfs

"The communication system . . . is the nervous system of science; the system that receives and transmits stimuli to its various parts." (Cole/Cole 1973, 16).

"Science today is an enormous depository of disconnected information." (Refinetti 1989, 584)

Contemporary concepts of science start from the *discursive* and *cooperative character of modern sciences*: Individual scientists are in this view merely small "wheels" in the overall "machinery," who make modest contributions to the progress of their disciplines. They stand on the "shoulders of giants" (Newton, Merton), i.e., on the basis of the methods, concepts and accumulated knowledge of generational chains of scientists, but also on the shoulders of "dwarfs": According to Solla de Price (1974: 13) 80-90% of all scientists who ever lived are alive today. Of course we do not yet know, which of the present-day "dwarfs" will turn out to be long-term "giants," that is theories, concepts, results will last, gain ascendancy.

Sciences are thus *collective undertakings* (Koller 1985, 390): their institutional networks, their media (languages), the procedures for reaching relatively secure and useful results are methodically regulated procedures of a genuinely *discursive* nature. The work of individual scientists has meaning and purpose only insofar as the results of their efforts are tested and criticized, introduced into other studies, combined with other individual analyses into syntheses in which common knowledge stocks can be increased, etc. Obviously rational, open, critical, functionally effective communication relationships between scientists are assumed, as "homini informatici," who continually wish to inform other scientists and are able to optimally inform themselves.

2 "a jungle of data, a desert of concepts" - Concepts and models of scientific communication research

Scientific communication could, as Shaugnessy remarked, be simply defined as "the social phenomenon whereby intellectual and creative activity is transmitted from one scholar to another" (1989, p. 69) - one of the typical formulations which after all (not very reality congruent) implies *dyadic* relational structures. As many authors have determined, scientific communication is, however, a complex process with a *system* character. The conceptual definitions or explications approach the topics from various levels or dimensions (especially media, filtering processes, scientific outputs) and differ especially in terms of whether they (a) are limited to formal scientific communication through journals with peer review or (b) also draw in *informal* forms of research communication. Electronic forms of scientific communication are only partly mentioned or focused on in the various conceptual definitions or explications; if they are then they are usually counted among informal forms.

A large share of the relevant research is particularly theory-less. Diane Crane (1970, 28) diagnosed: "For the most part, studies of formal communication and information gathering have been conducted in the absence of all but the most rudimentary theoretical models." Le Coadic (1987, 144) reduced the current situation to the formula: "a jungle of data covering up an underlying desert of concepts." Numerous "models" of scientific communication lack any recognizable theoretical foundation; they limit themselves concretely to enumerations of *individual components*, *respectively media of scientific communication*. It is thereby apparent, among other things, that not a few of these models, although called *systems*, de facto represent linear *chains*. What elements these models contain obviously depends on the professional provenance of the respective authors: thus, e.g., authors from library science assign the greatest significance to libraries, books and journals in paper form, insofar as other media or communication forms are at all mentioned; computer scientists, to the contrary, focus (as is to be expected) primarily, if not exclusively, on computer networks and database systems.

In scientific communication research we also find models borrowed from formal, natural, and engineering science which consequently rest on analogies, on the adoption of models from successful, "exact" sciences (a practice quite usual in other areas). They limit themselves almost exclusively to modelling the quantitative development of scientific communication or respectively of individual components/bearers:

(a) *Physical* models of information *diffusion* start from the assumption that the diffusion of scientific and technical information is analogous to the *diffusion of heat in solid bodies* (Avramescu 1973) and that therefore Fourier's law of heat transmission is applicable to information. The reader's interest (measured in terms of citations) corresponds to the potential (temperature), the accessibility of information (dependent on the circulation of the journal, language, niveau and style of the respective article) corresponds to the transmission capacity of the material, the diffusion space consists of the articles on a topic (connected through citations/ references, Avramescou 1975).

Derek J. de Solla Price 1974 (historian of science with a "certain prehistoric past as a physicist", loc cit. 9) compares science metaphorically with gas, i.e., the methods of quantitative scientific research developed by him with those of thermodynamics, in which the behavior of a gas is discussed under various pressure and temperature conditions: "If one stays with this metaphor, my first lecture deals with the volume of science, the second with the rapidity of their molecules' diffusion, the third with the interactions and the fourth with the derivation of political and social qualities of this gas." (loc cit. 10 f.)

(b) Technical models of information transmission (transmitter-message-recipient models) are based on the mathematical information theory of Shannon/Weaver, which was expressly not developed to study semantic information and therefore is generally employed in a way "alien to its purpose." Usually one connotation of this model is thereby overlooked, namely "the military idea of authoritative, directive and unidirectional transmission of orders." (Le Coadic 1987, 146)

(c) In the literature there are numerous variants of *biological* models of information diffusion with varying degrees of complexity:

In simple "infection" (epidemiological) models a formal analogy is drawn between the diffusion of common colds (e.g., flu, sniffles) and the diffusion of information in a population of scientific researchers; direct personal communication is thereby usually focused on (and not written communication).

Goffman/Warren 1980 present two- to four-step biomedical models of the transmission of infectious diseases. In the case of diverse tropical diseases three- and four-step transmission processes are to be observed, appropriate models have been developed to mathematically model their spread: Parasites need a temporary intermediate host for their development or transmission. Scientific journals are thus analogous to the function of mosquitos in the spread of malaria or of water snails in the further development of schistosoma (bilharzia, trematodes), the causes of hilharziose (schistosomiasis).

The previously sketched formal models have in common that as a rule they merely provide formulas for quantitative developments (e.g., growth in journals), but are hardly suitable as content or pragmatic approaches.

(d) Besides analogies from natural science and technology, *economic analogies* are also found in scientific communication research. Beniger 1988 draws the analogy *citation* = *money*. Both he understands as generalized exchange media in the sense of Talcott Parsons: "Both money and citation constitute symbolic systems that translate status across social contexts." (loc cit., 24). The author criticizes especially current one-way models of the information stream: the (feared by Beniger) modern "*control crisis*" of the sciences is based on the usually implicit assumption "that science primarily consists - at its most macro level - of a one-way informational flow: knowledge is created, processed or refined, communicated and utilized, possibly to create still more knowledge. Informed by this model, much computerization of information systems slights the reciprocal or feedback signals by which scientific outputs are controlled. Such feedback, perhaps most familiar in the form of scientific citations, but also as reputations of journals, editorial decisions, and a wide range of other such signals, does not represent knowledge produced but does confer status and authority differentially upon knowledge producers" (loc cit., 22 f.; Herv.G.F.) However, from a pragmatic perspective, status knowledge is also knowledge: to know who is powerful in a specific field is highly relevant for action.

As well a series of other authors emphasize the social differentiation based on status and authority in science and scientific communication. They differ fundamentally in the evaluation and attribution of the causes of these social disparities. While authors who are associated with the library sciences and scientific scientometry (especially of natural science provenance) often view these status differences unproblematized as an expression of the differences in the quality of scientific works - and not seldom equate resonance (frequency of citation) and quality of a work, authors located in the field of tension between epistemology and social sciences see social disparities or power structures in the sciences: recognition of authors is not necessarily an expression of the scientific quality of their publications. Concepts which also thematize the connection between power and information will in the following be sketched very briefly.

3. Power and information: Sciences as social action fields

3.1. Insiders and outsiders: Sciences as social figurations (Elias)

The German sociologist and cultural philosopher Norbert Elias emphasized people's mutual dependency (interdependence): They form figurations, dynamic relational networks, metaphorically comparable with the - admittedly still too static - line-ups of chessmen on a board, or the constantly changing constellations of social dances (e.g., quadrilles). Sciences are, viewed from a process- or figuration-sociological viewpoint, networks, relational structures, figurations of people who (in part voluntarily, in part without their will) are connected with one another, in a multifarious, multi-leveled and often opaque manner, *mutually interdependent* - not least importantly on the basis of a division of labor or functional differentiation: Behind concepts, theories, research designs and results are people or human groups engaged in interaction with one another.

Power is not a thing, not a special problem of, say, a special sociology, but rather a structural characteristic of all social relationships (Norbert Elias (1984)). The source of power is the control of (action) resources which others need. Important basic figurations are therefore those between insiders and outsiders. As well in the sciences numerous insider-outsider figurations can be determined: Thus, scientific progress occurs in a dialectic of *scientific establishments and scientific outsiders*. Innovations are produced mostly by outsiders, while the establishments often satisfy themselves with "normal science" (T.S. Kuhn) and with the codification of knowledge. A great share of the today posthumously famous and highly honored philosophers and scientists were in their lifetimes unheeded, even ridiculed and derided outsiders - and often had to atone for their "premature" (Bloch) concepts, inventions or discoveries, which overstepped the "truth" of the discourse of their epoch (Canguilhem, Foucault), with exclusion from the scientific discourses of their time.

Norbert Elias, contrary to the established theory of science in the singular, developed a theory of science in the plural (cf. as an introduction Fröhlich 1991). He also locates unjustified power hierarchies within the overall structure of the sciences - as for example when (classical) physics is presented as the ideal for all other sciences or mathematization as the scientific criterion per se. To the contrary Elias advocates and defends the relative autonomy of the individual sciences.

3.2 The symbolic capital of reputation: Sciences as fields of competition (Bourdieu)

The French social scientist and epistemologist Pierre Bourdieu represents the social world in the form of a multi-dimensional space: The actors or groups of actors are defined on the basis of their relative position within this space, and to be sure on the basis of (according to volumes and structure in the course of time) different action resources (capital). This space can also be described as a *field of forces*, "as an ensemble of objective relationships of forces which imposes itself on everyone who enters the field as a compulsion." (Bourdieu 1985, 10).

The individual fields of the social space are "historically constituting playing fields with their specific institutions and each its own functional laws" (Bourdieu 1992, 111). They are not just *gravitational fields* (loc cit., 72), but also "battle fields on which a struggle is fought for the maintenance or change of force relationships" (Bourdieu 1985, 74) The fields thus "need" acting people: Interest or illusio, the economic and psychic occupation of the respective game are simultaneously prerequisites ("insofar as it 'drives people', makes them run, compete, fight" Bourdieu 1992, 112) and products of the functioning field.

Capital (in the sense of accumulated work, both others' and own) represents "power to dispose in the frame of a field" and is equivalent to "*trumps in a card game*" (Bourdieu 1985, 10). Within the individual, relatively autonomous fields respectively different sorts of capital are in circulation. Besides economic capital, Bourdieu distinguishes cultural and social capital, as well as symbolic capital as a perceived and recognized form of these three sorts of capital: (cf. as an introduction Fröhlich 1994): Bourdieu differentiates three forms of cultural capital: assimilated, i.e., corporally bound (educational) capital in the sense of internalized, permanent dispositions or abilities (e.g., scientific knowledge, scientifically relevant skills in the sense of "tacit knowledge" (Polanyi)); objectified cultural capital (e.g., books, machines) - its use demands internalized cultural capital; institutionalized cultural capital in the form of educational titles. Social capital is the totality of the resources based on membership in groups, relational networks. Construction and reproduction of this relational or obligational capital demands continuing relational work (in the sciences, e.g., citations, positive reviews, small talk at conferences, the exchange of papers). Symbolic capital is based on familiarity and recognition (status, prestige, reputation) on standing out, on *distinction* from others; one could also regard it, it might be added, as the highest form of social capital.

In Bourdieu's conception , sciences are social fields, too - as well here it is a matter of the *accumulation of capital*. However, in the sciences it is not primarily a matter of the accumulation of material capital, but above all of *symbolic capital* (reputation, honor, prestige, distinction, recognized originality), inseparably connected with the struggle for *scientific credibility*: "As a system of objective relations between positions already won (in previous struggles), the scientific field is the locus of a competitive struggle in which the *specific* issue at stake is the monopoly of *scientific authority*, defined inseparably as technical capacity and social power, or, to put it another way, the monopoly of scientific competence, in the sense of a particular agent's socially recognised capacity to speak and act legitimately (i.e. in an authorised and authoritative way) in scientific matters." (Bourdieu 1975, 19)

In the scientific (as well as in in the artistic) field *competitors* are the consumers and critics of their own products - and conversely. The mutual control of competitors (Polanyi) demands, at least in the natural sciences, knowledge or reason. Bourdieu hopes for progress in the social sciences through a furthering of scientific critique, especially of *transitive critique* (Polanyi) instead of pair-wise quasi-ritualized ignoring, admiring or feuding (transitivity means here in simplified form: A criticizes B, B criticizes C, C criticizes A).

3.3 The privileging of the known("the more, the more"): the "Matthew effect" (Merton)

"For everyone who has will be given more . . . and everyone who has nothing will forfeit even what he has." (Matthew 25, 14-30, "the parable of the talents" Revised English Bible, 1989)

Robert K. Merton, the North American founder of the social study of sciences, has dealt with various (at least on first glance) irrational or dysfunctional processes in scientific communication. In his study of (what he called) the "Matthew effect" he thematizes the *"injustice*" of the scientific establishment in regard to the reward of scientific achievements. The "Matthew effect" is based on social disparities and strengthens them, e.g.: (a) If two scientists publish a study together, one already known or "reputed" and a less well-known scientist, almost all the attention and rewards are paid to the one who is well-known (e.g., honors, invitations, publication offers, research funds); (b) if two scientists independently make similar discoveries, the laurels likewise fall almost exclusively to the one who is better known.

The Matthew effect is not limited to the individual actor; as well with institutions (e.g., "renowned" US universities vs. less "renowned" ones) such cumulative privileging effects are to be observed, which again have effects on their members: scientists from renowned institutions are privileged over against scientists from less well-known institutions for qualitatively approximately equal research achievements.

Merton draws on the testimony of non-suspect witnesses, Nobel prize winners, thus privileged scientists; resentments were thus to be excluded. These beneficiaries of the Matthew effect noted with astonishment "that known scientists receive disproportionate recognition for their achievements, while relatively unknown scientists receive disproportionately little recognition for comparable achievements." (Merton 1985, 152)

As a structural functionalist, Merton focuses on the functions of the parts in maintaining the overall structure. The way such gratification mechanisms function, seemingly contradicting the achievement principle, is accordingly: famous scientists are easier to remember, they advance the "*visibility*" of theories. To Merton it appears obvious that as well in the sciences *information reduction by means of personalization* is unavoidable, not least of the scientific information explosion, which "discourages" (Merton) individual scientists. Merton's prognosis from the 60s: On the basis of the flood of publications the "Matthew effect" would inevitably intensify, since scientists, "in view of the difficulties of recognizing the significant works in their area, will search for external indications of which works they should devote their attention to." (loc cit., 159)

The paper he published in the well-known natural science journal "Science" (Merton 1968) led to numerous follow-up studies in the most diverse areas. Merton (1988) himself concluded 20 years later that as a whole his concept had been frequently confirmed, not only in scientific research, but also in other areas.

3.4 Fraud and deception in the sciences

Numerous studies of court rulings show a great number of unambiguously proved cases of *fraud and deception* in the sciences, not seldom in the most renowned institutions (e.g., Harvard University, MIT) cf. Broad/Wade (1984). According to the authors, one of the most important results of their study is that the established scientific publication system and the established mechanisms of scientific communication by no means live up to their self-chosen aspirations. They have above all failed to successfully uncover cheating in the sciences. Only a few of these cases of "deviant" scientific behavior have been uncovered by the established control mechanisms of scientific communication (above all: evaluations by anonymous referees). The previously detected cases were as a rule uncovered through interaction processes outside of established formal scientific communication (above all through a personal denunciation). Usually these accusations were ignored for a long time or attempts were made to cover them up. As well, everyday scientific practice appears consequently to rest primarily on trust (Coleman, Luhmann) and less on "organized scepticism" (Merton).

3.5 The manifacture of knowledge, science as rhetoric

The manifacture of knowledge is, according to the results of field studies by Karin Knorr-Cetina (1984) in natural scientific laboratories, even in the "hard," "rigorous" natural sciences, a process of trying, puttering, "stumbling," discoveries by chance, through a "chance-driven success logic" on the basis of "local knowledge." Massively deviating from the actual research process, this is portrayed in publications as a rigorous, deductively derived, universal process: In publication it is a matter of depersonalization, of the staging of relevance, the "literary construction of scientific rationality" (loc cit., 200). Natural scientists thereby pursue, among other things, the following strategy (understandable given the competitive situation): they publish (only) as much as necessary so that their claim (their assertion of priority of discovery, invention) can be demonstrated, i.e., as little as possible in order to make it difficult for competing research groups to rapidly replicate and continue the studies and overtake their own research team. Also, within the research laboratory researchers attempt to protect information leads through secrecy, information blocking.

This view of scientific communication as *rhetoric* is one of the most rapidly expanding research domains of the last few years (cf., e.g., Simons 1989). Its supporters start from the assumption - against philosophical justification fundamentalism and other objectivistic orthodoxies in the various disciplines (Simons 1989, X), that scientists behave rhetorically: "rhetoric is the form that discourse takes when it goes public . . . that is, when it has been geared to an audience, readied for an occasion, adapted to its end. . . . Rhetoric is thus not 'pure' information-giving, 'pure' logic, . . . though it need not be false to fact, illogical. . . . Rhetoric is thus a pragmatic art; its functions those of symbolic inducement. . . . The rhetor, said Burke, exploits the 'resources of ambiguity in language'" (loc cit.)

4 Inherent surplus values of scientific communication

Rainer Kuhlen (1991) understands information science as a theory of the creation of informational surplus value. In German-language telecommunications the concept of "value-added" has clearly become established, above all for services which go beyond mere network functions (even time-of-day provided over the telephone can be understood as a "value-added service"). Besides this in the information science literature there are various further types of informational added values or value increases (cf. likewise Kuhlen 1991): the greater information value of electronic media as opposed to conventional, inherent added-value effects (by means of improvements in individual components of already existing systems or of a systematic improvement of overall system performance). Value-adding effects also occur through conglomeration (e.g., universal hosts, gateways) and through integration (e.g., multi-media).

As a philosopher of science, Erhard Oeser (1986) understands science as an information system. He summarizes three concepts of science - (a) scientific knowledge products (statement systems), (b) methodically organized activity and (c) the producers of science (scientists and scientific institutions) - under the concept of *"information system":* "Under the concept of 'information system' is understood therefore not only the technical facility, but rather a specific functional quality which applies as well to the abstract system of 'science', specifically the quality of organizing communication processes."(loc. cit., 242)

From a heuristic view, consequently, we will analyze below less the surplus values of individual scientific information, information types, texts and other representations, but rather *the inherent surplus values of scientific communication "in itself,"* on the basis of its specific structural attributes compared with the communication structures of other fields. These functions or processes can be designated as the surplus value of scientific communication, because they - insofar as they succeed - add value to scientific information. Above all the following positive functions can be counted among the inherent (potential) surplus values (better: utility) of scientific communication: (a) stimulation of ideas, sources of motivation, encouragement to invest time, energy and "reputation" in a specific direction, in other words, "orientation," i.e., to hold to what is current, "what is in," means actually permitted as research topics; (b) avoidance of unnecessary redundant inventions, synergy effects, at the same time the intensification of competition; (c) support of argumentative security on the one side, of critique on the other or "evaluation"/quality control/selection.

We can view as *surplus-value reducing*, i.e., as costs of these forms of social control: (a) arbitrarily-selective, convergent information adoption, i.e., innovation-limiting conformism in the selection of research topics, employed theories, models, methods, (b) the stimulation of the publication explosion, "intoxication" with primary quantitative evaluative criteria ("publish or perish"), (c) "information frustration" and (unproductive) substantive redundancy as a result of over-publication, (d) the performance-hostile "injustice" of the Matthew effect, (e) inadequate functional competence in the uncovering of fraud and deception.

In the representation of the positive functions of scientific communication in the literature, *harmony of interests* is usually assumed It is overlooked that - as with information in general - the value or surplus value of information/ communication is only determinable *system-relatively* (or group-/subject related): *There is no perspective-free surplus value of scientific communication*. One can distinguish as configuring perspectives, e.g.: the (fictive) standpoint of the overall society (??), the perspectives of "extra-scientific" subsystems (economy, politics), the system of science as a whole, individual disciplines, schools/paradigms/communities, universities, departments etc., status groups of scientists or scientists depending on status or subject position, above all the scientific (epistemological) positions they represent. At the same time the functional competence of current systems of scientific communication is usually assumed to be relatively unproblematic. Only to a limited degree can one presently assume a "free flow of information" and transitive critique. The innovative, stimulating, synergistic, etc. potentials of freely flowing communication and critique are at present - so we could summarize section 3 - at any rate only partly realized.

Following Pierre Bourdieu surplus value of scientific communication could be thought of as the accumulated "symbolic capital" of individuals, groups and institutions, their acquired claim to scientific credibility as surplus value. These action resources are unequally distributed and (see above) not necessarily exclusively indicative of achievements: where capital already exists (especially social or symbolic), more is easily added.

This sketch is obviously too general. It is necessary to differentiate value- or surplus-value dimensions (knowledge value/action value/symbolic value) and levels or dimensions (media, situations) of scientific communication, above all the different functions of various forms of scientific communication (e.g., informal research communication vs. formally controlled scientific communication: the first stimulates ideas, and also performs the initial filter functions, the function of the second is less the furthering than the control of the sciences).

The significance of *unplanned* processes of communication or information should be taken into consideration. Niklas Luhmann (1974) suggests the *promotion of chance events*: "No central distribution of relevant information can be set up or effected through decisions, because the knowledge of needs cannot be centralized. Nor can it, which is less well-known, be required that the individual scientist create his own information, because neither can knowledge of need be individualized. Very often need is first shaped by information which the individual obtains "by chance;" frequently without looking for it the individual researcher finds information which is useful, appropriate or discrepant, inspiring, fills gaps, facilitates the creation of abstractions, and we cannot get along without this form of communication. Neither central distribution nor individual searching are in themselves sufficient methods. It appears rather that scope for chance events must be provided in the steering mode of the system, which must be distributed densely enough to make possible higher expectations of success. There must, in other words, besides good search opportunities also be relationships, groups, places, readings, etc., in which people are exposed to densely enough distributed unsought information." (loc cit., 236)

Furthermore, different inherent surplus value/surplus-value expectations are foreseeable depending on the individual scientific discipline. Certainly scientists of different disciplinary provenance harbor different expectations in regard to the surplus value of scientific information: Not a few theoretical social scientists or humanistic scholars value in texts and authors the productive ambiguity, multifacetedness, associative stimulus potential, transfer, contextualization, motivational, entertainment values, productive reduncancy (in the sense of parallel formulations in several different language games/reference systems), "genealogical surplus values" (indications of similarity or relatedness among concepts/theories/solution strategies), export/import functions (transparadigmatic, transdisciplinary translation values) - while advocates of so-called strict disciplines reject such "vague" concepts and "inconsistent" formulations under certain circumstances as "unscientific." In general, it is certainly the case that *context-sensitive* representations (e.g., semantic networks) offer considerably more informational surplus value than context-poor ones, for example, in the form of alphabetical listings. Informational surplus value is consequently structural, contextual.

But we should also observe the communication resistant (hence surplus-value reducing) behaviour of scientists: Indices also make possible, e.g., more efficient control by competitors, rapid demonstration of errors. As a consequence of strategies to immunize against critique (motto: "the book as fortress") - it is my subjective impression - the share of books with indices is declining, even though the effort required to generate them has decreased thanks to support by electronic text processing systems. Among the strategies to immunize against critique may also be the exceeding of speaking time limits at conferences (in order to reduce discussion time), but also the intentionally excessively vague or unintelligible formulation of scientific texts, among many other possibilities. The defense strategies of scientists can thus hinder the realization of potential informational surplus values.

5 surplus value realization/distribution across subject information systems and computer networks?

How could we realize the potential surplus value of scientific communication in data base systems and computer networks or value-added services? How could, on the other hand, social disparities be reduced, in other words: how could the distribution of (symbolic) surplus values of scientific communication be organized in order to reward scientific achievement?

In recent literature there are expressions of both hope and pessimism regarding the effects of "*telescience*," i.e., the telematization of scientific communication utterances. Thus Aborn 1988 and Beniger 1988 assume that the development of electronic scientific communication is shaking the foundations of classical, formal, controlled scientific communication, as "distancing of researchers from the very process of science, . . . a shift away from modes and norms that have characterized scientific communication in the past." (Aborn 1988, 11). Beniger 1988, on the basis of the spread of modern information technologies in the sciences, even diagnoses a "crisis," a "control crisis" of science: "Telematics threatens global science. . . . [with] . . . a crisis of control. Many involved with the computerization of information systems have predicted - some gleefully - a decline in the formal scientific paper, a blurring of the distinction between research notes and papers and between papers and the response to them by others, an increase in multiple authorship by scores or even hundreds who participate in a telematic discussion, and the decline of formal journals, editors, and the gatekeeping function more generally." (loc. cit., 26 f.)

Some authors find above all in the "anarchistic" Internet (cf. Fröhlich 1995, 1996a) reasons to hope for a "democratization" of scientific communication. What does this mean in the present context? Besides the dampening of the "performance hostile" Matthew effect it could also mean the furthering of transitive scientific critique, beyond barricaded "invisible communities" with their exclusive information distributors, "citation cartels" and complimentary reviews which reduce the equality of opportunity in access to scientific action resources and the diffusion of work on the basis of quality as opposed to merely the author's name recognition.

Reduced costs and therefore enormous accessibilities of scientific production - and communication technology are certainly potentially democratizing. However, initial user studies show that the networks are primarily used frequently and successfully by persons who already have personal relations to other researchers - i.e., those who already have social- or symbolic capital (face-to-face relationships at conferences, invisible communities, citation cartels, etc.), to them will be given (rapid and economical use, intensification of these relationships through electronic connections over any given geographic distance) - thus rather a reinforcement than a weakening of the Matthew effect is to be predicted.

On the current state of information technologies and especially of information *methodologies* (and decisive competencies of the scientists) the effects of data banks and computer networks will entail not an unburdening, but rather a further *intensification of the flood of (redundant) information* (and thereby an increase in information frustration), not least of all due to the greater visibility of the information flood. Erhard Oeser (1986) fears that "the production of non-relevant information increases the more effective and convenient information channels become" (loc cit., 254) and focuses on the function of science as an information transformation and destruction machinery: One should also build into modern information systems "the possibility of ignoring or forgetting information." (loc cit.).

In order to develop the potential of data base systems and computer networks and to use the various strategies of informational surplus value formation (see Kuhlen 1991) effectively, one cannot rely on the inner dynamics of information technologies. Necessary are institutional reforms in the domains of scientific research, teaching, publication, information. Reforms to improve the exploitation of the potentials of the networks and data base systems should, among others, include the following steps:

(a) Methods of systematic information acquisition, filtering, reduction and concentration, as well as of creation/use of informational surplus values should be intensively further developed and introduced into all study programs and the further education of scientists. The increasing rational and efficient use of great quantities of information, for example by means of value-added network services, could contribute to a furthering of equality of opportunity in regard to the use of scientific action resources.

(b) The quality of database systems themselves could, among other things, be increased through a "learning" of systems through error elimination, evaluation, cross-references by users. Conceivably, e.g., users could be rewarded for error reports with free search time, limited point quotas could be allocated per user (plus- and minus points) to evaluate publications, own commentary files could be set up for the individual documents, comments on cross references, links could be established to other databases: with other words, conventional online data banks could learn from internet habits.

(c) The improvement of intersubjectively testable oversight work (state of the art, clearing-house activities) is indispensable. Both in the East (Michailow et al.) and the West (e.g., Refinetti 1989) information experts demand the creation of scientific information managers: "A whole class of information managers is necessary to perform the highest function in the progress of knowledge - namely, the integration of disconnected data into a coherent whole" (Refinetti 1989, 584). Such integration work and contextualization requires, however, above all theoretical efforts: concept-, hypothesis-, theory formation can be understood in terms of the philosophy of science as steps in the reduction resp. concentration of information (Oeser 1976).

(d) Our current scientific system is primarily a *publication* system and not an optimal communication system: Through their publications researchers accumulate symbolic capital, points on their application forms. The numerous strategies of immunization against critique and the information blockade would have to be opposed with a new ethics of scientific information and communication, the pressure for quantity in the sense of "publish or perish" as well as the Matthew effect should be mitigated by the creation of new criteria of evaluation and new reward criteria resp. reward structures of scientific communities. However, the chances of realizing such reforms cannot be viewed in an optimistic light. Furthermore, it is questionable whether a shared, objective position can be created (perhaps based on a common interest, such as "dominance-free communication" in Habermas' sense), and it is uncertain how we should deal with configurative surplus-value perspectives; there is a danger that they will be seen merely as interference variables - and not as an "objective reality of perspectives" (G.H. Mead 1969, 213)

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